

Flight From Behavior Analysis Presidential Address ABA 1980

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It seems appropriate for a presidential address to take the form of a "state of the union" message. Such a message usually consists of telling the good news and who should receive credit for it; the bad news and who is to blame (this second part is usually the longest); and then what we can do about it. The ending is usually a little weak since it is pretty hard to do anything about most bad news. My "state of the union" message will have much this same general pattern.

The Good News

The good news is that in a period of only forty years the field of behavior analysis has shown remarkable growth. Figure 1 portrays the development of the field of behavior analysis since the Thirties, in terms of available texts, journals, and organizations. The heavy emphasis on books and journals reflects my own perspective as a teacher, but in general the strength of a field is somewhat related to the ease with which the relevant knowledge can be transmitted, and this is closely linked to the availability of written material that can serve instructional purposes.

Prior to 1930 there wasn't much going on. Of course, Watson and others had begun the behavioral movement, but the critical distinction between operant and respondent relations was lacking and

everyone was trying to interpret operant processes and relations in terms of respondent conditioning, and not getting anywhere. In the period from 1930 to 1938, Skinner put it all together. He managed, within that brief but fertile period, to come up with almost all of the essential methods, concepts, and functional relations of our field as we see it today: a focus on the behavior of the single organism, rate of response as the main dependent variable, the cumulative record, the operant-respondent distinction with the related difference between the CS and the S^D, and the effects of various kinds of intermittent reinforcement. The only technique that was lacking was shaping, and that was discovered a little later.

But *The Behavior of Organisms* was a difficult book, and Skinner's descriptive science was incomprehensible to the bulk of learning psychologists in that heyday of the Hullian hypothetico-deductive approach. The field grew primarily as a result of Skinner's own personal influence at Minnesota and Indiana, and Fred Keller's at Columbia College, where he started the first undergraduate program in behavior analysis in 1946. This consisted of a two-term introductory course with laboratory (which was the model for the many subsequent introductory laboratories) and six more courses. This undergraduate program, shown in Figure 2 as it functioned in 1949, played a major role in providing behaviorally trained graduates to Columbia and to other doctoral programs.

In 1947 the first Conference on the Experimental Analysis of Behavior was held at Indiana University. Subsequent conferences of this series gave rise to the

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Figure 1.
Development of the Field of Behavior Analysis in
Terms of Teaching Materials and Organizations

- 1938 *Behavior of Organisms* (B. F. Skinner);
Fred Keller takes position at Columbia College
(the operant-respondent distinction and the S^D distinguished from the CS, single
subject methodology, rate of response as the dependent variable, the cumulative
record, effects of various types of intermittent reinforcement, effects of depriva-
tion and emotional variables, etc., etc.)
- 1945 First behavioral undergraduate program, first introductory course with an operant
rat laboratory—Columbia College, Fred Keller; First Conference on the Ex-
perimental Analysis of Behavior; William James Lectures (Skinner)—circulated in
mimeographed form; *Walden Two* (Skinner)
- 1950 *Principles of Psychology* (Keller and Schoenfeld)
Science and Human Behavior, (Skinner)
Learning: Reinforcement Theory (Keller)
- 1955 *Verbal Behavior* (Skinner) *Schedules of Reinforcement* (Ferster and Skinner),
Journal of the Experimental Analysis of Behavior (JEAB)
- 1960 *Tactics of Scientific Research* (Sidman), *The Analysis of Behavior* (Holland and-
Skinner), *Cumulative Record* (Skinner)
Child Development I (Bijou and Baer)
Complex Human Behavior (Staats and Staats)
- 1965 *Child Development II* (Bijou and Baer); APA Division 25; *Case Studies in Behavior*
Modification (Ullmann and Krasner); *Operant Behavior Handbook* (Honig, ed.)
Control of Human Behavior I (Ulrich, Stachnik and Mabry); *The Analysis of*
Human Operant Behavior (Reese); *The Experimental Analysis of Behavior* (Ver-
have, ed.); *Child Development Readings* (Bijou and Baer, eds.); *Principles of*
Behavior Analysis (Millenson); *Journal of Applied Behavior Analysis* (JABA);
Behavior Principles (Ferster and Perrott);
- 1970 *Technology of Teaching* (B.F.S.); *A Primer*
of Operant Conditioning (Reynolds); *The*
Token Economy (Ayllon and Azrin); *Con-*
temporary Research in Operant Behavior
(Catania, ed.); *Contingencies of Reinforce-*
ment (B.F.S.); *Behavior Modification in the*
Natural Environment (Tharp and Wetzell);
(By 1970 the publication of texts on some
aspect of behavior analysis was increasing
to the point that it is not possible to list
them on a figure such as this one. There are
hundreds of items that deserve mention
and any selective mention would unjustly
offend more people than it would please.
Instead I show only Skinner's works, some
handbooks, two new journals, and a new
organization.
- 1975 *Beyond Freedom and Dignity*
(B.F.S.)
Behaviorism (A new journal)
About Behaviorism (B.F.S.)
- Midwest Association for Be-
havior Analysis (MABA—
later ABA)
- Reflections on Behaviorism and*
Society (B.F.S.); *The Shaping*
of a Behaviorist (B.F.S.);
Handbook of Operant Behavior
(Honig and Staddon, eds.);
The Behavior Analyst
(journal); *Handbook of Behavior Modification* (Leitenberg, ed.); *Handbook of*
Applied Behavior Analysis (Brigham and Catania, eds.)
- 1980

Society for the Experimental Analysis of Behavior (SEAB) which is responsible for publishing the *Journal of the Experimental Analysis of Behavior* (JEAB) and the *Journal of Applied Behavior Analysis*

(JABA). *Walden Two* was published in 1948 and in 1950, Keller and Schoenfeld's very influential introductory text, *Principles of Psychology*, came out.

Skinner's *Science and Human Behavior*

Figure 2.

The Undergraduate Major in Psychology at Columbia College Around 1949

Introductory Psychology: A two-term course with lecture and laboratory, directed by F. S. Keller.
Laboratory experiments performed during the course: (Keller, 1977)

1. Operant conditioning with regular reinforcement.
2. Retention and extinction of a conditioned operant.
3. Periodic reconditioning (at fixed intervals).
4. The formation of a discrimination.
5. The reversal of a discrimination.
6. The effect of punishment.
7. The reduction of operant latency ("reaction time")
8. Chaining
9. Secondary reinforcement
10. The effect of drive upon response rate
11. Light aversion
12. Conditioning an avoidance response
13. The conflict of motives
14. An experimental prototype of "fetishism"
15. An experimental prototype of "masochism"

Discrimination: A one-term course with lecture and laboratory, taught by W. N. Schoenfeld.

Motivation: A one-term course with lecture and laboratory, taught by W. N. Schoenfeld.

Conditioning: A one-term course with lecture and laboratory, taught by F. S. Keller.

Abnormal: A one-term lecture course taught by R. F. Hefferline.

Senior Seminar I: One-term seminar, taught jointly by Keller, Schoenfeld, and Hefferline. (various topics, e.g. verbal behavior, emotion, etc.)

Senior Seminar II: One-term seminar, taught jointly by Keller, Schoenfeld, and Hefferline. (various topics).

(Two courses, Social and Differential, were at this time still offered with a traditional content, but a few years later were combined with the Abnormal into a two-term behavioral course called Behavioral Socialization and taught by Hefferline.)

appeared in 1953 and was, it seems to me, the main factor responsible for the development of the area called behavior modification. Though all of the basic principles had been available in *The Behavior of Organisms*, and were later available in a more easily understood form in Keller and Schoenfeld, the development of the behavior modification movement needed Skinner's own bold extrapolation to all aspects of human behavior. Most experimental psychologists are inherently conservative in describing the relevance of their work to practical situations, but not Skinner. In *Science and Human Behavior*, using only the basic concepts of behavior analysis that appeared in *The Behavior of Organisms*, some results of his subsequent work with pigeons, and the material which ultimately went into *Verbal Behavior*, he managed to deal with a wide variety of human situations from a completely behavioral point of view, and very convincingly at that. It was this extension to all aspects of human activity that, I think, provided behaviorists with the encouragement necessary for them to begin contributing to the areas of mental illness, mental retardation, and other applied fields.

Keller's pamphlet, *Learning: Reinforcement Theory* came out in 1954, and was probably the first abbreviated version of behavior analysis suitable for a brief course or a workshop. *Verbal Behavior* and *Schedules of Reinforcement* both came out in 1957, and the first volume of the *Journal of the Experimental Analysis of Behavior* (JEAB) was published in 1958. It was soon possible to have an undergraduate curriculum based entirely on behavioral texts and journals (although even now there are very few such programs in existence). Sidman's *Tactics of Scientific Research* was published in 1960: We now had our own book on research methodology. Holland and Skinner's programmed text, *The Analysis of Behavior*, which made it

possible to teach our subject matter more efficiently, appeared the same year.

In the Sixties, texts were beginning to be available in specifically human areas, such as child development—Bijou and Baer's first two volumes plus their *Readings*, Staats and Staats' *Complex Human Behavior*, and perhaps most significant of all, Ullman and Krasner's *Case Studies in Behavior Modification*, which facilitated the development of a "generation" of behavior modifiers. By 1966 the literature in the field was so extensive that it became valuable to have a handbook, Honig's *Operant Behavior: Areas of Research and Application*. The American Psychological Association's Division for the Experimental Analysis of Behavior (Division 25) was started in the Sixties; likewise the *Journal of Applied Behavior Analysis* began publication in response to the increasing number of applied papers being submitted to JEAB. Skinner continued to forge ahead with *Contingencies of Reinforcement*, where he articulated the very important distinction between rule-governed and contingency-shaped behavior, analyzed the interactions between the phylogeny and ontogeny of behavior, and provided a further analysis of the role of private events, among other things.

By the early Seventies the contributions had become so numerous that they cannot be portrayed on Figure 1. Perhaps it would be well to mention just a few more recent books indicative of our growth: The Honig handbook has been updated in the form of a new handbook, edited by Honig and Staddon (but with a significant shift in emphasis, as discussed later); there are now at least two handbooks of applied behavior analysis (Leitenberg, 1978; Brigham and Catania, 1978); and of course Skinner continues to further enrich our available source material.

In terms of advances other than texts, we now have a journal (*Behaviorism*) devoted to theoretical and philosophical aspects of the field; several new journals

devoted to the applied areas; and of course, the Association for Behavior Analysis (ABA) which provides another new journal, *The Behavior Analyst*, and an annual 5-day program so packed with presentations that the complaint is heard that too much is going on at the same time.

I have portrayed our growth from the perspective of a college teacher, in terms of the things that can be used to "transmit the behavioral culture" to new members of that culture. There are, of course, many other signs of growth. Many more people are now working in the field. A number of graduate training programs are mainly behavioral in orientation. The number of conferences on various aspects of behavior analysis every year is already high and continues to rise. There are no major areas of human endeavor that do not have behavior analysts as respected participants. And so on, and so on.

The good news, then, is that there are now many more of us and we are actively involved in many important contributions to our society and to humankind in general. From the narrow perspective of the teacher, the good news is the very large number of articles, books, journals, and other materials now available for instructional purposes and the large number of students interested in learning. From this same narrow perspective, the bad news is that these highly desirable materials seem to be influencing a smaller and smaller proportion of the increasingly large number of people working in the field. Actually, it is somewhat more complex than this, and as with most state-of-the-union messages, this bad news must now be considered in detail.

The Bad News

To explain my discontent, it is necessary to describe further some of the salient features of the field during its early period of rapid growth. This, of course, is not the bad news, but rather the condition with which later developments will be contrasted.

Behavior Modification

In the late Fifties and early Sixties the people involved in what came to be known as behavior modification were mostly academicians, who had, I believe, much the same orientation as my own. In the fall of 1955 I had just received my Ph.D. from U.C.L.A. and was teaching in the Psychology Department at Kansas University. My graduate training had been mainly in experimental psychology, statistics, and philosophy of science. I was teaching an introductory course and while looking for some reasonable lecture material stumbled onto *Science and Human Behavior*. In the span of a few hours I was converted to Skinner's point of view. *Science and Human Behavior* was not only the most convincing and detailed analysis of human behavior that I had seen, but it also implied a world view that I found very attractive. The book left me with the strong impression that the most important thing I could do with my life was contribute to the further development of the science of behavior and promote behaviorism as the principal basis for dealing with human problems on an individual, but more importantly, on a broad cultural level. There was a kind of desperate temporal aspect to this enthusiasm also, in that we had to move quickly because there might not be much time left. I still have this sense of time running out.

My interest in the applied field was, to some degree, forced on me. I was a teacher and much of my teaching consisted in talking about the science of behavior as described by Skinner. I lectured and argued about it in my own classes, argued with my colleagues in psychology, gave talks to university groups outside psychology, and to groups outside the university. The message of all this verbal behavior was essentially that we were in a lot of trouble and behaviorism was the only way out. Most of the graduate students at K.U. in psychology at that time were in the

clinical program, and so I was often challenged to conceptualize practical problems in behavioral terms. How would Skinner deal with schizophrenia, with mental retardation, with the adult neurotic? What are the implications of Skinner's approach for music therapy (which was big at K.U. at that time)? What would a behaviorist say about education, about industrial and business problems? And so on. The answers to such questions were readily available to me in *The Behavior of Organisms*, *Science and Human Behavior*, *Walden Two*, Keller and Schoenfeld's *Principles of Psychology* and in my mimeographed copy of Skinner's William James Lectures on verbal behavior. With this background there were no topics that I couldn't deal with, at least as well as they were being dealt with by others. My behavior was well maintained by what I saw as successes in these interchanges, and it also interested students, although I never convinced any of the cognitive psychologists in the K.U. Psychology Department. (This is K.U. well before Don Baer and company arrived at the Department of Human Development.)

When I moved from K.U. to the University of Houston (at the request of the Kansas tenure committee—I won many battles, but lost the war) I was further drawn into applied work by my association with Lee Meyerson, a leading figure in the psychological aspects of physical disability and illness (presently represented in the APA by Division 22). He was a cognitive psychologist (in the Lewinian sense—not in the information processing sense) and was at that time much concerned with sensory deficits, particularly among the mentally retarded. Lee and I became good friends, each trying to convert the other to his own theoretical point of view, and at one point in one of our arguments he asked, "If you know so much about behavior, how would you test the hearing of a severely retarded child?" Well, by that time

Donald Blough had published several papers in which he described a method for studying the pigeon's visual sensitivity, so I could answer that it would be easy. We got a research grant and collaborated in the development of an audiometric technique using operant conditioning. It wasn't all that easy, but worked out pretty well nevertheless. This was my first experience with applied research, and as a result of this work with Lee Meyerson I soon became further involved in mental retardation, physical disability, and other applied areas. I knew that there was no better way to deal with problems in these areas than behaviorally, but my real goal all along was to contribute to the science of behavior and to "behavioralize" the culture. It was clear that people working in those areas should learn about the science of behavior as quickly as possible. This work in behavior modification was valuable in its own right, but for my purposes it was mainly a way to show people how valuable it was to know about behavior analysis and behaviorism, and thus move the behavioral revolution a little further along.

Illustrative of this general orientation were my priorities in the area of elementary education. It was clear early in the game that it would be valuable to improve the teacher's ability to control children's behavior in the classroom. If teachers knew more about contingency management, they would be better able to teach, and we could and should help them in this respect. Likewise, the increasing knowledge of programming had important implications for improving the effectiveness of instructional material. But neither of these possibilities was as important for me as another, which still remains to be accomplished: to add the science of behavior to the elementary school science and social studies curricula. This would produce a population which is much more receptive to behaviorism and its implications for the design of the culture.

During this early period of rapid

growth people entered and contributed to the field of behavior analysis in various ways, but most of the ones I met and talked with were either graduate students in psychology programs or college teachers in such programs. Their background in behavior analysis consisted in knowing as much about behavior as one could find out from studying the texts and articles available at that time—refer to Figure 1. These academicians were generally quite familiar with this body of knowledge, and in addition they viewed behaviorism as more important for the human species than just as a particular approach within psychology. Perhaps the most salient features of this behavioral orientation were these: 1) a heavy emphasis on the environment as the source of behavior; 2) a skillful interpretive use of the principles

and facts of behavior, which meant the ability to provide behavioral interpretations of a) everyday events, b) concepts and theories of various nonbehavioral psychologies, c) ethical and moral issues, and d) various aspects of scientific methodology; finally 3) they had some understanding of the complex role of private stimuli and responses in behavior analysis, as a result of Skinner's *Psychological Review* article (1945) or of Chapter 17 in *Science and Human Behavior*.

I may be exaggerating the significance to others of the themes that I valued very much myself, but it is at worst only a slight exaggeration. When I left Arizona State University in 1967 and came to Western Michigan, I found a completely behavioral undergraduate psychology ma-

Figure 3.
The Undergraduate Major in Psychology at W.M.U. Around 1970

Course	Credits	Textbooks
150 Psychology I: An Introduction to the Science of Behavior (with laboratory)	3	Whaley and Malott, <i>Elementary Principles of Behavior</i>
160 Personality and Developmental Psychology	3	Lundin, <i>Personality: An Experimental Approach</i> Bijou and Baer, <i>Child Development I and II</i> ; also <i>Readings</i>
250 Behavior Modification I: Abnormal Behavior	3	Ullmann and Krasner, <i>Case Studies in Behavior Modification</i> .
260 Behavior Modification II: Normal Behavior	3	Skinner, <i>Verbal Behavior</i>
350 Analysis of Behavior I: Stimulus Control of Behavior (with laboratory)	5	Millenson, <i>Principles of Behavioral Analysis</i> ; also Sidman, <i>Tactics of Scientific Research</i>
360 Analysis of Behavior II: Contingencies and Consequences (with laboratory)	5	Honig, <i>Operant Behavior: Areas of Research and Application</i>
450 Methodological Foundations of Psychology	3	Games and Klare, <i>Elementary Statistics</i>
460 Systems and Theories	3	Marx and Hillix, <i>Systems and Theories in Psychology</i>

jor in which I could participate by teaching about verbal behavior. This major is shown in Figure 3 as it was functioning around 1970. At that time this seemed the proper training for a major in psychology, and also the appropriate background for a person entering the masters degree program.

A Shift in Emphasis

In the early Seventies the field began to shift its emphasis away from behavior analysis as a basic science and behaviorism as a broad basis for understanding the human condition, toward behavior analysis as a profession and behavioral psychology as the basis for a relatively specific technology. This shift, I think, resulted from four developments, none undesirable in themselves, but which in combination, ultimately had a clearly detrimental effect.

New personnel. First, the background interests and orientation of people entering the field began to change. The early behavior analysts were attracted to the field as a scientific discipline and were, in a sense, a minority group within their academic setting. They were behaviorists among many who were not. They pursued their intellectual interests in spite of considerable opposition from others—they were clearly not a part of the academic “establishment.” As students they had either acquired an interest in behaviorism from their reading, or had been influenced by “maverick” professors who were themselves in opposition to the more conservative eclectic majority. The term “commitment” seems very appropriate in characterizing the relation of these early behavior analysts to the science of behavior and to behaviorism as a world view. But as the field grew, people who had quite different perspectives began to enter it. Quite a few were primarily interested in helping people (not *People*, but rather individual persons), and they saw behavior analysis as a good way to do this. There are still some looking for a

better understanding of the human condition, but many of the current students are primarily looking for a reasonable occupation. They become behavior analysts simply because that is what is taught at the university that is nearby, although this is not to say that such students don’t sometimes become “committed” to the field.

Also, as behavior modification began to be seen as an effective technology, a number of eclectic applied psychologists already working as clinicians, school psychologists, industrial psychologists, etc. began to add behavior analysis to their collection of techniques, much as earlier eclectic academicians added Skinner’s views to their intellectual repertoire. And while they may well have acquired the technology, these new professionals did not generally acquire the science or the philosophy of science that was responsible for the technology.

Almost anyone can succeed. A second development, quite positive in itself, has led to some difficulties in the relation of the independent variable to the principles of behavior. This is the development of a powerful research methodology within the applied field itself, which could be learned with out much knowledge of the principles of behavior and with which one could be quite successful in effecting important changes in various applied areas. Early behavior modification studies simply transferred animal laboratory technology to the applied setting, even to the point of enclosing the subject in a small chamber and using lever pressing as the response. Or they made use of whatever methodology was available from traditional applied research. Neither approach was entirely satisfactory. What was needed was more attention to the measurement of the dependent variable, as well as some new experimental designs. In basic operant research a dependent variable like lever pressing or key pecking is picked for its recording convenience. In the applied field one must deal with the dependent variable that defines the pro-

blem, which usually involves some relatively complex behavior that can't be defined instrumentally or in topographical terms. This has increasingly required human observers, and observer reliability then becomes a major problem. Also, the standard reversal design of the animal laboratory is inapplicable in many applied situations, and new ways of demonstrating experimental control had to be developed. By 1968 much of this methodology was becoming available, largely due, I think, to the work of the people in the Department of Human Development at Kansas University—Baer, Wolf, Risley, and their colleagues, although of course others also contributed.

Furthermore, the new methodology was well taught by those who developed it, well described and illustrated in our new publication, the *Journal of Applied Behavior Analysis*, and could be learned and practiced without any knowledge of basic research methodology, without much knowledge of the principles of behavior, and certainly without any commitment to behaviorism as a world view. With these research strategies a person could be quite successful in many applied settings. I certainly don't wish to characterize this applied methodology as undesirable, but it did lead to an undesirable shift in attitude toward the independent variables. All one had to have to produce noticeable improvements in many applied settings was a methodology with regard to the dependent variable—a good way to obtain relevant data—and an elementary understanding of the importance of behavioral consequences. One could even do without the latter, since with a good dependent variable you can try anything as an independent variable and eventually succeed in some respect. In the early behavior modification studies the independent variables were always closely linked to the principles of behavior as they had been developed in the animal laboratory: reinforcement, extinction,

punishment, stimulus control, and so on. It was important to the researchers to establish the continuity of their work with these general concepts. More recently, however, this continuity has become less important, and independent variables are increasingly described in common sense terms, or in terms that refer to omnibus procedures which are only roughly similar across applications, such as time out, feedback, response cost, overcorrection, etc. Also contributing to the decline in the use of general behavioral concepts is the outcome-cure orientation's encouragement to manipulate simultaneously anything that might work. This often results in "package" independent variables of such complexity that they simply can't be analyzed into basic behavioral components, especially when they involve highly verbal subjects. In summary, there are more and more people working in the field who either can not or will not relate their manipulations to the general concepts of the science of behavior. And there are also editors of *JABA* who accept this new style of applied science.

Demands of the marketplace. A third factor that began to be important in this period was what could be called the demands of the marketplace. Most of the growth in opportunities has taken place in the applied areas, and the people who take such jobs soon come under the control of the practical needs and demands of those areas. One such demand is for a measurable outcome of one's professional activity, in terms of clients cured, students educated, people returned to work, etc. Our ex-students are increasingly working in settings where they must be able to show what they have accomplished for the clients, the taxpayers, the pupils, and the state. It is not sufficient, nor required, that they contribute to the generality of existing behavioral principles, or further the cause of behaviorism. This means that practical outcome becomes more important than knowing about the way

variables affect behavior. Component analysis is not necessary, and if costly or time-consuming it is actually a hindrance, at least with respect to short-term consequences. One is encouraged to manipulate any variable that can be easily manipulated, irrespective of its rationale with respect to the principles of behavior.

Another demand of the marketplace is for skills other than behavior analysis. When hired in an applied situation our people soon find themselves required to perform many functions unrelated to the title of their graduate degree. Figure 4 shows some of the areas of skill and knowledge that are relevant to a large portion of the behavior analyst's daily work activities. In terms of what many of our M.A. and Ph.D. graduates do in the applied areas we might have prepared them better if we had skipped behavior analysis entirely and developed a curriculum based on Figure 4. We can't go this far, of course, but as a result of pressure from those who hope to enter such job settings and those who are already there, we have

introduced more and more of these other topics into our curriculum—and of course, have dropped some of our behavior analysis courses to make room.

Many academicians with primary interest in the applied areas also find themselves functioning more as administrators than as behavior analysts. They too become very involved in managing their graduate assistants, secretaries, and other staff, in grant writing, legal and ethical issues, evaluation research, and so on. Since such academicians need help with these activities they are often sympathetic with students' interests in practicing such activities while they are still students. And unfortunately, taking courses, studying textbooks, and writing papers on behavior analysis all get in the way of what to these administrator-academicians is more useful work.

Much of this other activity does not require a knowledge of behavioral principles nor a commitment to behaviorism. In fact, the features that characterized the early behavior modifiers may now be con-

Figure 4.
"Essential" Topics for Success in Applied Behavior Analysis

Accounting

Program Budgeting

Management by Objectives

Systems Analysis

Market Analysis

Consumer Satisfaction Research Methodology (opinion-attitude questionnaires)

Evaluation Research (now necessary as a part of most grants)

*Legal Issues (Clients' rights, licensing, and just to understand what has been
"mandated" by the state or federal government)*

State Mental Health Systems

Federal Law in Health and Human Services

Grant Writing

Staff Training

Computer Usage

Influencing Others (winning friends, influencing people, dressing for success, etc.)

Public Speaking

*(By now I'm sure there are several more such topics, each requiring a course or two
for thorough understanding.)*

sidered "counterproductive." For example, they say it is not good public relations to talk about things that your clients or associates may not understand: things like "reinforcement contingencies," "schedules of reinforcement," "stimulus control" (Drop "control" in general—it causes a lot of trouble.), and "punishment" (Don't ever use this term!). Such talk doesn't help your relations with people (now often referred to as "folks") who may have some control over the success of your various projects and activities. It is presumably much better to talk to everyone in language that is easily understood. Technical terms are seen as just a form of jargon that is easily dispensed with. Likewise, a commitment to behaviorism may seem to be a hindrance. After all, you certainly don't want "behavioral fanatics" running around trying to talk the folks out of their mentalistic view of human nature while you're trying to convince them of how much they need you. You certainly don't need to confront the mentalism of your client, they argue, or client's direct care worker, or the administrator of the unit, in order to change the relevant behavior.

So the need for a curriculum loaded with the principles of behavior and behaviorism becomes questionable. Of course, I have no question about the need for such a curriculum, but it is questioned when the focus is on short-term payoffs, to the neglect of important longer range goals.

Mass dissemination. A final development interacts with the previous one in furthering a shift away from science and philosophy toward technology and professionalism. In our efforts to disseminate our subject matter we have often found it necessary to reduce its extensiveness and its complexity. We find it useful and important to provide instruction in behavior analysis to teachers, aids, parents, physicians, and other groups who have little relevant background knowledge and little time to spend on this education. There is

nothing wrong with this. Our field is not the sort where a little bit of knowledge is dangerous. A little bit is clearly better than none. So we have boiled our subject down to the barest essentials: a one-semester course, a 6-hour workshop, even more commonly a 3-hour workshop. We prepare pamphlets that explain "how to do it" in simple language, and this is a reasonable way to accomplish a worthwhile task. The problem arises when we begin to believe that this is all there is to the field, when our boiled down materials become the basis for undergraduate college instruction, or even graduate instruction. Unfortunately, the abbreviated curriculum fits right in with the demands of the marketplace discussed above. How convenient that we can instruct in behavior analysis with a course or two, since there isn't much room left in the curriculum after we put in all the other things.

So, to summarize, the bad news is that many people working in the applied field no longer have a strong background or much interest in the science of behavior, nor have an understanding or commitment to behaviorism. But why is this bad news? Much is still being accomplished, perhaps more than ever before. What are the harmful effects of this shift?

Our Own Black Scorpion (Verbal Behavior, p. 459)

Basic versus applied. At the 1977 conference of the Midwest Association for Behavior Analysis (MABA, now ABA) there was a symposium titled "Experimental analysis and applied behavior analysis: Reconciliation or divorce?" (Note 1) Several argued for divorce. Before discussing these arguments it seems appropriate to suggest that by analogy with other sciences and technologies, the very occurrence of such a symposium suggests that we are in trouble. Imagine a similar symposium where the relation between medicine and biology is being considered, or between engineering on the one hand and physics and math

on the other. The notion is preposterous. It is possible that the analogy doesn't hold, but I hardly think that we are that unique.

Speaking in favor of divorce some symposiasts asserted that it was no longer necessary for people in the applied areas to learn anything about basic research methods and results. They had their own science, in terms of a methodology quite appropriate to their needs; and the results coming from basic research, especially with lower animals, were not worth the trouble—which was considerable—that it took to understand them. Another symposiast argued that there had never been a marriage anyway. Basic research had never been aimed at any particular application, but was instead conducted for what could be called the “aesthetic” interests of the basic researcher. It was a form of intellectual challenge and was most valuable when pursued in this spirit. Any serious concern with applicability would ultimately work against scientific productivity, and the two fields should keep their distance for the benefits of both.

Before considering the arguments for reconciliation it is necessary to say a few words about some changes that have taken place in basic research since *JEAB* started publication in 1958. Many of the early studies extended and elaborated techniques of behavioral control. Animals were found to be sensitive in many ways to the details of complex contingencies. Their behavior could be brought under the control of a variety of stimuli and relations between stimuli, and could conform to rather stringent time and number requirements. This type of research had obvious relevance to human behavior, and when the efforts at control failed, this too seemed clearly related to similar failures in the control of human behavior.

I began using *JEAB* as a text for a seminar in behavior analysis in 1958, and have used it off and on ever since. The students typically obtain the current

volume and one or two back volumes and we read and discuss what seem to me to be the most important articles or the articles that have some special pedagogical relevance. Over the years I have seen a shift in emphasis in *JEAB*. Now there are relatively fewer articles that simply extend the technology of behavior control, and relatively more articles concerned with theoretical issues such as the matching law, and with the interaction of operant relations with the kind of behavior originally of interest to ethologists. Experiments seem less often to be derived from previous efforts to control behavior—previous work by the same experimenter—and more often devised to test some hypothesis derived somehow from theory and from the previous results of others (The new handbook edited by Honig and Staddon reflects this same shift.). In this respect *JEAB* is becoming more like the *Journal of Experimental Psychology*, except for an emphasis on experimental rather than statistical control. This, of course, is not a trivial exception and is partly responsible for the ease with which research results can be used to refine our general understanding of behavior and can be extended to the human condition. Still, although it may be a weakness on my part, I find it more difficult to so refine and extend on the basis of the work in theory testing and ethological relations. I suspect that my difficulty in this respect is not atypical, and these changes may be in part responsible for the applied scientists' disenchantment with the basic science. But in spite of this shift in emphasis there are still more articles published each year that are of potential value to the applied behavior analyst than we can cover in our one-semester seminar.

The arguments for reconciliation generally consist of a description of losses to both the applied and basic areas that result from their isolation. The seriousness of this problem can be seen from the fact that several excellent

treatments of the same or related issues have recently appeared in print (Dietz, 1978; Hayes, 1978; Birnbrauer, 1979; Pierce and Epling, 1980; Branch and Malagodi, 1980).

One major loss to the applied field concerns the lack of generality of findings when independent variables are not closely related to basic behavioral concepts. With an outcome or cure orientation rather than an analytic or investigative orientation, the independent variable, as described earlier, tends to become whatever is necessary to get the job done. Typically such independent variables are complex environmental manipulations such as the presence or absence of a parent-consequence system of support for a child's school work, self- versus experimenter-designed behavioral contracts, lecture versus videotape presentation versus modeling as a way of learning how to tutor retarded children, and so on. It may be important to manipulate such omnibus independent variables, although the resulting knowledge doesn't seem to cumulate well in the sense in which science and technology are said to be cumulative. But unless such variables are analyzed by the original investigator in terms of more general concepts (like reinforcement, stimulus control, stimulus generalization) transfer of such results to novel situations is a matter of trial and error, and transfer failure often unanalyzable. A good example of this situation is the confusing picture resulting from the numerous studies manipulating "feedback" as an independent variable (Schwade, Note 2). Feedback is a prime example of a term referring to a collection of only roughly similar procedures. Most of the studies using this variable consisted in telling someone how they did on some task, or how their behavior affected someone else. It is not surprising that the results of manipulating such a variable are not consistent. Such a stimulus will sometimes function as conditioned reinforcement, sometimes as conditioned punishment, sometimes as a

discriminative stimulus, and sometimes as both consequence and discriminative stimulus. Often, as will be discussed later, the behavior responsible for the feedback occurred minutes, hours, or days before the feedback was given, in which case any effect as a consequence must have been due to some complex change in the subject's verbal behavior which in turn resulted in some further change in nonverbal behavior. I can't imagine that there will ever be a science of feedback in this sense. Birnbrauer (1978) gives another example illustrating the same difficulty. He describes two studies, both using an independent variable that could be called "imitating undesirable behavior." In one a child was a sloppy eater, and when the teacher ate sloppily in the child's presence, the child's eating improved. In another the child "yelped" as a form of inappropriate vocal behavior, but when the teacher yelped in response, the child's yelping increased. So what do we conclude? Does this represent an important exception to some general principle that should be investigated further? Who knows what it represents? The only way such results could be used is in some sort of computerized catalog of findings, where you enter with all the parameters of your own situation and search for studies with similar parameters. Such a "data bank" has been proposed as a way to deal with our expanding "knowledge," and while this may well be useful it seems to me to be neither science nor technology, but rather a form of simple empiricism resorted to when situations are virtually unanalyzable. I think the science of behavior has come far beyond this stage by now.

Paralleling the loss to technology that results from loss of contact with its basic science, there is a loss to the basic science that is just as serious. The applied field is an important testing ground for the generality and sufficiency of the principles and techniques discovered in the laboratory, especially since much of the

laboratory work is done with lower animals. Unsuccessful application suggests undiscovered complexities, the identification of which is especially important as we extend our analyses to more complex human behavior. Likewise, successful application functions as a form of support for further work along certain thematic lines. But when the independent variable is "whatever can be manipulated," the results neither question nor support any aspect of the basic science. Just as important, at least to some of us, the simple empiricism that has become increasingly widespread is neutral with respect to world view, or even general scientific orientation. Successful "cure" by manipulation of "package" or "omnibus" independent variables offers no support for behaviorism—in fact, it seems to me that increasingly such "packages" contain frankly mentalistic or so-called humanistic components, seemingly as a result of the experimenter's having run out of ideas for behavioral manipulations, or having an affinity for such components because of insufficient behavioral training. We certainly won't behavioralize the culture this way.

Another loss for the applied field that results from insufficient contact with basic research is not being able to take rapid advantage of basic research findings that are highly relevant to various aspects of applied work. For example, much applied work with the retarded consists in developing stimulus control. The training paradigms are usually the direct descendents of laboratory techniques that continue to be studied and about which much new information is available. A great deal more is now known about conditional discrimination than when such procedures as matching-to-sample were first extended to applied settings, but very little of this new information has been made use of in such settings. Likewise, the technique of stimulus shaping for developing control by complex or subtle stimulus features (Schilmoeller,

Schilmoeller, Etzel, and LeBlanc) is still virtually unknown in the applied field. Recent developments in what is called feature value research, and the seeming ubiquity of blocking as a factor in the development of stimulus control are also highly relevant to much practical work, but workers in the applied field make little or no use of these findings. That some of this basic research is being done with human subjects makes it especially transferable to applied settings, and makes ignoring it especially inappropriate.

Superficial behavior analysis. Another harmful effect of the decreased emphasis on the science of behavior is a type of analytic superficiality, a decreasing ability to interpret human behavior in terms of the complex interacting variables that so often are relevant. The basic research literature, whatever its other faults, does constitute an example of sophisticated interpretation. Even what seem like fairly simple experimental arrangements involving lower animals often turn out to be interpretable only in terms of complex interactions among several variables whose quantitative parameters play a significant role. By contrast, many human situations are often tossed off as examples of this or that simple concept, when in fact such analyses are quite superficial and easily spotted as such by critics of the behavioral approach. For example, a common human problem is procrastination which then necessitates intense last-minute activity. Students put off studying for exams until the last minute and then "cram," often not very effectively. This common human situation is often described as a fixed interval scallop, with the seeming implication that it is to be understood in terms of our existing knowledge of fixed interval reinforcement schedules. There is, of course, some kind of fixed interval that is relevant, and behavior increases near the end of the interval—thus resembling a scallop, but the relevance of fixed interval reinforce-

ment schedules is so remote as to be nonexistent. An accurate behavioral analysis of such procrastination followed by intense activity is quite complex, involving control by competing sources of reinforcement with limited holds, avoidance, external time and work correlated stimuli, and several other factors, which to some extent differ from situation to situation. To refer to such complexity as a fixed interval scallop is a form of superficial nonsense. In the case of the student studying for the exam the superficiality is often compounded by the notion that the test is the reinforcement for the studying that takes place. This general approach is understandable in the case of the undergraduate student taking an introductory course in behavior analysis, and is reminiscent of such students' tendency to interpret industrial pay by the hour as a form of fixed interval reinforcement. But when such shallowness characterizes the verbal behavior of the professional behavior analyst, it is clearly bad news.

An even more serious form of superficiality is to ignore the role of rule-governed behavior in supplementing defective contingencies. Many contingencies, possibly most, that are arranged or analyzed in work with the normal adult or child, and especially in the field that is coming to be known as organizational behavior management, are defective in the sense that the consequence occurs far too long after the relevant behavior to have any direct effect on that behavior. An example is the person who quarrels with a spouse in the morning before going to work and then, to restore what is perceived as a somewhat damaged relation, brings home a gift for the spouse that evening. It is not uncommon for a behaviorally oriented person to advise against such gift giving on the grounds that it will only reinforce quarreling. But even on the face of it this is not reasonable. Such a gift could not possibly function directly as reinforcement for the

behavior that took place six to ten hours earlier. And even if it did have some direct effect on this behavior, much larger effects would be seen on the behavior that was located closer in time to the consequence, such as all the things that were done that afternoon. This is a clearly defective contingency, and it can only alter behavior through some relatively complex form of contingency description or rule stating on the part of the person receiving the gift. But if what people say to themselves about such events—if the rules they derive—play an important role in the ultimate effect of the contingency, then we must consider it equally possible that a misrule will be stated, or that a rule will be derived that works in the opposite direction from that of the defective contingency, in which case the behavioral psychologist's prediction will be quite wrong. Instead of saying to oneself something like "I guess I don't have to worry about pressing my position in arguments even if it makes him/her angry . . ." the spouse might say "Sweet thing! S/he felt bad because of the quarrel this morning. S/he really loves me and I shouldn't quarrel so over things which are trivial in the long run," and they might live happily ever after. In which case would we want to say that contingencies of reinforcement don't work, or only work some times? A colleague of mine who had gone to some trouble to write a grant proposal, on receiving notification that the grant would not be awarded remarked that that was punishment for grant writing on his part. But the grant writing occurred five months earlier. The only way such an event could affect future grant writing is through a quite complex collection of verbal processes, which are not really well understood at this time, although Skinner has made an important beginning in dealing with such issues (1957, p. 357-367; 1969, p. 146-171).

In organizational behavior management people are frequently given "feedback" regarding their work, or given

bonuses for certain types of accomplishments—in a simple case, coming to work on time. Such manipulation is often described as a form of contingency management, and often has favorable effects. But in almost all such cases the behavior being affected is too far in time from the consequence to be affected directly. Such effects are probably always mediated through some form of rule statement or rule control, which is typically not mentioned or analyzed. And as mentioned above, contingencies that succeed by a rule may fail by a rule, and often do, in individual cases. With the current generally unanalytic orientation, however, one comes to accept occasional failure rather cavalierly. After all, with an outcome orientation the main point is to get more of the folks to come to work on time and if our procedures don't work for everyone they may still be worth implementing. We can always use the analysis of variance to prove that we're accomplishing something.

Creeping mentalism. As our graduate programs cease generating a thorough and aggressive form of behaviorism our graduates are increasingly insensitive to mentalistic encroachments into the field and do not constitute a verbal community functioning to provide mutual support and criticism. Their behavioral approach can become so diluted that it is really a form of eclecticism, and while they may be better liked by their nonbehavioral colleagues their effectiveness in dealing with behavior is severely limited. An example of such creeping mentalism is the increasing popularity of the notion that "an SD (discriminative stimulus) is a stimulus that *signals* reinforcement." This phraseology is much more suggestive of an organism that is processing information than one whose behavior is being controlled by a stimulus. Another example is the widespread acceptance of the terms "receptive language" and "expressive language" as ways of distinguishing between the effects of verbal stimuli on a

listener and the verbal behavior of a speaker. The implication that both speaker and listener are in some sense dealing with the same thing, namely language, is not compatible with any detailed analysis of verbal behavior and it encourages the view that language acquisition consists in learning the meanings of words. Another popular terminological device is to contrast structure with function, in dealing with language or with behavior in general, and then in the spirit of detente to suggest that structure is what cognitive psychologists are interested in and function is the realm of behavior analysis. This isn't detente. It's capitulation.

The general problems resulting from an insufficiently thorough behavioral philosophy are nicely dealt with by Branch and Malagodi (1980), and although they are concerned with the academic setting, the points apply equally well to many applied settings.

It wasn't so long ago that the spark of commitment to behaviorism glowed brightly. That spark is barely visible these days as repeated Mentalistic mic-turitions have dampened it. Mentalistic psychologists, against whom we were once so squarely pitted, have outwitted us. Behaviorists have largely failed to develop cohesive training programs within major Ph.D. granting institutions. With few exceptions (e.g. University of Florida, Western Michigan University, University of West Virginia), a single "token Behaviorist" usually finds him or her self isolated in a department composed of mentalistic psychologists and residing in diverse "areas" such as "social psychology," "developmental psychology," "learning," etc. In these environments, the fledgling behaviorist eventually succumbs to the reinforcement and punishment practices of the immediate verbal community. Individual fledgling behaviorists have usually found themselves surrounded by mentalists who eventually come to control the behavior of the poor former behaviorist. These mentalists pay lip service to some of our more powerful methods to demonstrate their open-mindedness, and then reinforce our open-mindedness in accepting mentalistic concepts. They take advantage of our pre-graduate school, excessively mentalistic history, and soon (often before the tenure deadline) the former behaviorist is acknowledging the central role of cognitions (i.e. mental events) in the determination of behavior. Their task has been made easier by the view that behavioristic procedures are best used as band-aids to fix bad behavior.

How have we let this happen? Are we doomed to

pursue the mentalistic path for the 297th time in history? This may be our last opportunity to address the issue if the world continues on its present course (p. 36-37).

So, there's the bad news: fewer generalizable independent variables in the applied area, decreased critique and direction from the applied field to those doing basic research, decreased sensitivity by applied workers to potentially useful basic findings, widespread superficiality of behavioral interpretations of human behavior (particularly with respect to defective contingencies and the role of verbal behavior), and finally the increasing dilution of the behavioral approach with mentalistic terms and concepts.

The Solution

Now that some of the problems have been identified, what's to be done about them? In one sense the answer is simple. The major faults are with the graduate training programs. They are failing to generate the repertoires needed in the area of behavior analysis. To correct the difficulty it is only necessary to improve such programs by increasing the depth of training in several essential areas. But our programs are already crowded with time-consuming training activities. It often takes our students five or more years beyond the bachelors degree to complete the requirements for the Ph.D. Where will we find the time for more instruction? I think we already have the answer.

Package the digressions. We have become very good at packaging abbreviated versions of behavior analysis for mass dissemination. This is what we should do with the other topics that are crowding behavior analysis out of our programs. Looking again at Figure 3, the behavioral psychologist is not expected to be a contributor in any of these areas. State and federal law regarding mental health, for example, may be an extensive subject, justifying several courses if one wanted to be a specialist in this field, but how much of this does the behavior analyst have to know? Probably just

enough to stay out of trouble and to talk with experts in the field when necessary. A self-instructional package taking from four to six hours should be more than sufficient. The same is true of most of the other areas listed in the figure. Even a topic like evaluation research, to which some people devote several graduate courses, doesn't justify that degree of time commitment if one's main interest is behavior analysis. All of this material, if properly abbreviated and packaged could possibly comprise a semester's worth of self-instructional units; then the remaining three and a half years could be spent learning behavior analysis.

There is a problem with this solution, however. Some students who find themselves in a behavior analysis program may not be all that interested in it. They may, in fact, find that systems analysis, grant writing, computer usage, mental health law, and other such topics are the things that really interest them, and they may prefer to spend their time with these topics. Our attitude toward such preferences should, I think, be the same as the attitude of a chemistry department toward a student who finds math and physics preferable to the study of chemistry. They "encourage" such students to leave the field of chemistry and pursue degrees in those other areas. It is our responsibility to generate effective repertoires in behavior analysis. Those who do not value such repertoires sufficiently to spend the time necessary to obtain them should not receive degrees in behavior analysis, but rather in their "chosen" fields of interest. We should be careful not to encourage that form of dilettantism that often passes itself off as interdisciplinary breadth. It is hard to be a specialist in law; but it is not hard to know more about law than most psychologists, and in this way be at least a sort of specialist. Such semi-experts can also escape evaluation by true legal specialists, for example, by stressing their behavioral expertise. I think some of our students are

attracted by exactly this possibility. It is generally fun to find out about new areas that were previously unfamiliar, but in terms of effort, time demands, and speed of progress, it is much more difficult to pursue such topics seriously. It is easy to be a dabbler in several areas, but fortunately there are few degree programs or professions that provide that opportunity. Let's be sure that behavior analysis doesn't become one of them.

A program in behavior analysis. What would the recommended more intense training in behavior analysis consist of? It would perhaps be better to describe the desired repertoire in terms of competencies, but I simply can't do that to my satisfaction at the present time. I can only identify it in terms of broad areas and in terms of the relevant teaching materials. With this "process orientation," then, training would be in three main areas, although there is some considerable overlap among them: basic science, applied science, and behaviorism. Figure 5 shows these areas in terms of the text materials used to teach them. There are presently a great many fine text materials in these areas, and if proper time were spent on such materials, people would learn a great deal, I hasten to point out that many equally fine books and materials could have been selected to represent these areas, and if your own work or some favorite work by someone else is missing this should not be taken amiss. I show here the texts and materials with which I am most familiar or have enjoyed using. There are many equally satisfactory alternatives. The listing is meant to indicate the extensiveness of the training, however. If you prefer other works, they should be substituted for those shown, but the total magnitude of the training material should not be further reduced.

Not shown on the figure is some treatment of the history of the field. There is no single text that deals adequately with this topic, most of them spending far too

much time on nonbehavioral approaches to human nature. This would probably have to be taught from a collection of articles or based extensively on lecture material. Another aspect of the training that is not shown is practice at behavioral interpretations of other points of view. Certainly a full semester course should be devoted to analyzing behaviorally the content of some introductory but thorough treatment of modern cognitive psychology, likewise for the material contained in a text on systems and theories, such as that of Marx and Hillix (1979).

It might be argued that such a program would take too long. But I am considering our field to be equivalent in extensiveness to the other sciences or to the engineering disciplines. To develop a doctoral repertoire in chemistry takes an intense undergraduate major of from 30 to 40 credit hours or more plus four years of graduate training. I think that it takes about the same amount of time to accomplish the same level of expertise in behavior analysis. A very good repertoire could, of course, be acquired by means of a good undergraduate major plus two more years of master's degree training.

Teach it better. A final aspect of my recommendations for reversing the trend away from the science of behavior and away from behaviorism concerns educational technology. To generate the repertoire implied by Figure 4 will require teachers who are at least more knowledgeable in all aspects of the various areas than the students they are instructing. This means that the instructor will have to spend a good deal of time reading, studying, making up exams, grading exams, and interacting with students regarding the subject matter. This is not something that can be easily delegated to a teaching assistant, however much that might enhance the repertoire of the assistant. This cannot be mass education, at least not the graduate components. It is widely recognized that laboratory and research training is time

Figure 5.

A Minimal Doctoral Repertoire in Behavior Analysis

- I. *Prerequisite background:*
 - The Analysis of Behavior* (Holland and Skinner, 1960).
A recent introductory text with extensive coverage of both basic and applied concepts and results.
Introductory animal laboratory experience.
- II. The science of behavior.
 - A. Basic:
 - Principles of Behavior Analysis*, revised edition (Millenson, 1978)
 - Selected chapters from *Operant Behavior* (Honig, 1966) and from *Handbook of Operant Behavior* (Honig and Staddon, 1978).
 - Many articles from current volumes of the *Journal of the Experimental Analysis of Behavior*
 - Laboratory experience (two or three courses) with rats, pigeons, and humans.
 - Science and Human Behavior* (Skinner, 1953) special emphasis on the last seventeen chapters.
 - Behavior Analysis of Child Development* (Bijou and Baer, 1978); *Child Development II* (Bijou and Baer, 1965); *Child Development: The Basic Stage of Early Childhood* (Bijou, 1976).
 - A recent text on behavioral sociology.
 - Verbal Behavior* (Skinner, 1957).
 - Tactics of Scientific Research* (Sidman, 1960).
 - Strategies and Tactics of Human Behavioral Research* (Johnston and Penny-packer, 1981).
 - B. Applied:
 - Several of the introductory texts on applied behavior analysis (and the fact that they all review basic principles to some extent and that they overlap with each other in coverage of applied concepts and results is a strength rather than a weakness).
 - Handbook of Behavior Modification*, (Leitenberg, 1978).
 - Handbook of Applied Behavior Analysis* (Brigham and Catania, 1978).
 - Many articles from current volumes of the *Journal of Applied Behavior Analysis*
 - Technology of Teaching* (Skinner, 1968).
 - Research Methods in Applied Behavior Analysis* (Bailey and Bostow, 1979).
 - Research experience in several different applied settings.
- II. Behaviorism:
 - About Behaviorism* (Skinner, 1974).
 - Contingencies of Reinforcement* (Skinner, 1969).
 - Walden Two* (Skinner, 1948).
 - Beyond Freedom and Dignity* (Skinner, 1971).
 - Cumulative Record* (Skinner, 1972) (Some articles are appropriate for II A and II B above as well.)
 - Reflections on Behaviorism and Society* (Skinner, 1978).
 - The Shaping of a Behaviorist* (Skinner, 1979).
 - A number of articles from the *Journal of the Experimental Analysis of Behavior* and from *Behaviorism*.

consuming, and best conducted as a form of apprenticeship. I think the more verbal aspects of the repertoire are just as deserving of time and attention from the instructional staff. In a sense what is needed is a form of intellectual or verbal apprenticeship. The students have to behave verbally and have their behavior reacted to by someone who thoroughly understands the issues. This cannot take the form of a midterm and a final exam, or a single term paper to represent the work of a whole semester.

So, there's the good news, the bad news, and the solution. What I'm recommending unfortunately involves more effort and time on the part of people who are already busy. It involves disregarding the preferences of many of the students in our programs, ignoring the opinions of many of our colleagues, and to some considerable extent ignoring the demands of the marketplace. But the field did get started under conditions where all these things were ignored because the potential gain was well worth the effort and social aversiveness. It still is.

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